

Letters to the Editor

November 4, 1968

Editor
Bacteriological Reviews

Dear Sir:

I am writing to protest the form and content of an "Addendum in Proof" which appeared in the review of G. Edlin and P. Broda in the September 1968 issue of Bacteriological Reviews. The "Addendum" states that the review was to have appeared in the June issue but was delayed because of my objections to their discussion of my work. It states further that the authors decided not to make any changes of substance in response to these objections. The authors thereby imply that the paper as it was published did not contain changes meeting some of my criticisms and that the delay of three months in publication was unnecessary. This letter will indicate that in the published paper the authors had made numerous changes in their discussion of our work and that their Addendum contains a gross misrepresentation of the facts. I wish this letter to be published in Bacteriological Reviews.

The paper of Edlin and Broda was sent to me in May 1968 marked "to appear in Bact. Rev. in June 1968." Since that manuscript contained inaccuracies and serious deficiencies in dealing with our work, I immediately wrote to this effect to the Editor, who wrote early in June that my letter of criticism had been sent to the referees and to the authors. I had heard nothing of the fate of the paper or of my criticisms until the publication of the paper in the Journal.

The authors have made the following changes in their paper, apparently in response to my letter. I had said in my letter to the Editors:

1. "Our hypothesis that the absolute level and ratio of polyamines control RNA synthesis did not stem from observations on appearance of polyamine in the medium, as stated by Edlin. It arose from observations on variations of intracellular RNA and spermidine, and stimulation of the former by exogenous polyamine."

The authors rewrote paragraph 1 in the section on Polyamines, p. 214, to delete references to appearance of polyamine in the medium and to indicate that spermidine accumulated in the cell, as does RNA.

2. In paragraph 2, Edlin and Broda have now introduced a reference to the observed inhibition of putrescine biosynthesis by spermidine, another point made in our letter.

3. All of paragraph 3 discusses a paper which the

authors had not seen prior to my calling it to the attention of the Editors. They have now added to their bibliography, i.e., Raina, Jansen, and Cohen, J. Bacteriol. 94:1684 (1967).

4. The authors had originally stated that "no measurements of the effect of polyamine-addition on protein synthesis were reported by Raina and Cohen." In fact this laboratory had reported on the effect of spermidine on aminoacid incorporation in the paper listed in 3 above, as well as in another paper to which Edlin and Broda had referred. Edlin and Broda have now added paragraph 5 in the section on Polyamines to discuss these data which they had missed.

5. In another section (paragraph 3, p. 217), the authors have now added a reference to our published data on the effect of methionine in stimulating spermidine biosynthesis.

6. In still another section on coordinate regulation of RNA synthesis, the authors have now added a reference to our paper of 1964, on "The synthesis of messenger RNA without protein synthesis," Stern, Sekiguchi, Barner and Cohen, J. Mol. Biol. 8:629 (1964).

It must be immediately evident that the authors have made "changes of substance" in their published paper, as compared to the version I saw in May. These changes were evidently made in response to my letter of criticism. It must then be asked why the authors were permitted to add remarks which attempted to suggest that their published paper was essentially unchanged. The Editors have an obligation to publish good papers and to hold up weak ones until the papers have been improved to their satisfaction, even if not to the satisfaction of individual critics, such as myself. In this instance, a paper was held up for three months and was finally published containing significant additions in response to criticisms which were in fact accepted by the authors. Is it the intent of the Editors from now on to permit critical referees to be held up for public obloquy because authors are piqued by short delays in publication despite corrections and improvements in their papers?

Sincerely yours,
Seymour S. Cohen

Nov. 12, 1968

Dear Sir:

I appreciate the critical attention which Dr. Cohen has given our review, a preprint of which we

sent to him, as well as to a number of other colleagues, after it had been accepted by the Editor and set in type. Our statement in the addendum, that "no changes of substance" were made in response to the objections which Dr. Cohen addressed to the Editor upon receiving our preprint, means that we did not change any of our *interpretations* or *conclusions*. Since it had been our, apparently mistaken, belief that Dr. Cohen's objections were mainly directed against these interpretations and conclusions, we thought it desirable to provide additional references to Dr. Cohen's cognate publications and to call special attention in the Addendum to what we thought were his disagreements with our views. We also wished to explain why our review was not allowed to go to press as scheduled. We did not intend to cause a "critical referee to be held up for public obloquy."

Gordon Edlin

Editor's Note

The "Addendum in Proof" to which Dr. Cohen refers was worded as follows: "This review was to have appeared in the June issue, but it was delayed because of objections raised by S. S. Cohen to the discussion of his work presented here. Since the authors decided not to make any changes of substance in response to these objections, readers are referred to a summary (14) of Dr. Cohen's own views regarding the regulation of RNA synthesis."

I accepted this Addendum, since it correctly explained the delay in publication of the review, and because it seemed a courteous way of calling the readers' attention to Dr. Cohen's opposing views. I was aware that corrections had been made by the authors in response to some of Dr. Cohen's objections, but was also aware that many of his other objections had not been answered. In particular, the authors did not change their interpretations of Dr. Cohen's published results. The statement that "changes of substance" had not been made thus seemed appropriate, and in any case the Addendum did not seem to me to offer offense in any way.

That Dr. Cohen took offense is sufficient reason for me to offer my apologies, since I have held him in affection and esteem for many years. It is important, however, that I correct the impression given by his letter that "critical referees (may be) held up for public obloquy because authors are piqued by short delays in publication. . . ."

Dr. Cohen was not a referee of the manuscript in question. As his letter indicates, his objections were provided gratuitously. I found them sufficiently serious, however, to delay publication until the authors and referees could consider them. In answer to Dr. Cohen's final question, it is certainly not my intent to permit referees, invited or otherwise, to be held up for public obloquy by piqued authors.

Edward A. Adelberg
Editor

18 November 1968

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I certify that the statements made by me above are correct and complete. (Signed) Robert A. Day, Managing Editor.